To the Editor:

I was disappointed in the article by Gabb and Robin (Hyperbaric Oxygen: A Therapy In Search Of Diseases. Chest 1987; 92:1074-82) because it was obviously not submitted for critical review before it was published. To have this article published is a discredit to the journal and an insult to readers. I do not intend to write a major critique of the article but wish to point out that as an attempt at historic review of hyperbaric oxygen it is not factual. As a risk-benefit analysis it lists and discusses risks erroneously (eg, decompression sickness is not a risk to patients), it does not show benefit or lack thereof factually or accurately (showing a remarkable lack of understanding of the references cited) and certainly is not an analysis of anything.

The most unfortunate result of the publication of the article is that it passes on misinformation to the readers of Chest. Hyperbaric oxygen therapy is presented as an unproven practice of medical adventurism when it is a well accepted practice for a short list of acute and chronic conditions. This misinformation may result in the rejection of hyperbaric oxygen therapy for some patients who could benefit, thus causing avoidable morbidity and mortality.

Dr. Robin appears to be a crusader against practices that he perceives result in iatrogenic morbidity. It is ironic that through his article he may cause iatrogenic morbidity because his misinformation will beget neglect of a beneficial therapy.

G. H. Koch, M.D.
Hyperbaric Department,
Toronto General Hospital,
Toronto, Ontario, Canada

To the Editor:

The Canadian government is pioneering the elimination of medical practices that are used on patients with a flimsy database to support their effectiveness. I am sorry that this process has not yet reached the Toronto General Hospital.

If HBO were enthusiastically accepted by all physicians, it would still be wrong to use it without an adequate database that established its risks vs benefits. Consensus approval or disapproval is not the point. Patient safety and effectiveness of treatment are the important issues.

When (or if) proponents of HBO provide such evidence, I promise to extol its virtues. Until then, I believe the best interests of patients are served by a rigorous criticism of the use of an unproven modality. Dr. Koch might hasten that day by using his chamber to provide such data in any one of the specific diseases he treats. Until he does, his opinions are that—simply opinions.

Eugene D. Robin, M.D.,
Stanford University Medical Center,
Stanford, CA

HIV Infection and Tuberculosis in Turkey

To the Editor:

Infection with human immunodeficiency virus (HIV) has been recently assigned as one of the reasons leading for an increase of tuberculosis in some populations. Whether a full blown AIDS picture develops or not, immunodeficiency occurs among infected individuals soon after seroconversion in HIV infection. Data which have been obtained suggest that the immunodeficiency caused by HIV infection may reactivate M tuberculosi s among populations which were widely infected with this bacterium (PPD positive). It is also suggested that HIV infection may be responsible for the increase of tuberculosis in these populations.

The incidence of tuberculosis has risen in our country in recent years. It has been estimated that there are 10 to 12 million individuals infected with M tuberculosis and about 200,000 patients with active tuberculosis in Turkey. Although only a few cases of AIDS have been reported from Turkey (34 patients as of November 30, 1987), we have investigated 221 active pulmonary tuberculosis patients and 38 cases who had experienced tuberculosis within the last five years in order to find out whether HIV infection contributed to this increment. These individuals were from 58 of 67 provinces in our country. Anti-HIV antibody was screened by a commercial enzyme-linked immunosorbent assay (Abbott Laboratories). None was positive.

This result has, at least at the present time, indicated that HIV infection does not contribute to the increase of tuberculosis in our country and implicated that HIV is not an indigenous human retrovirus in Anatolia.

Osman S. Yenen, M.D.; Erhan Ekinci, M.D.; Recep Aydilek, M.D., and Ibrahim Baydar, M.D., Gülhane Military Medical Academy, Istanbul, Turkey

REFERENCES

1 CDC. Tuberculosis and acquired immunodeficiency syndrome Florida. MMWR 1986; 35:587-90
5 Yücel B. Türkiye’de verem savasının göçmeliği agamalar (the steps in the fight against tuberculosis in Turkey). Türkükuloz Toraks 1986; 34:214-23

Erratum

To the Editor:

In our recently published paper, "Acute Postbronchodilator Changes in Pulmonary Function Parameters in Patients with Chronic Airways Obstruction", (Chest 1988; 93:541-46), several reference numbers were cited in error on page 544, last paragraph, and page 545, first column. References cited elsewhere in our paper are correct. The necessary corrections are as follows: 1) Page 544—citation of reference 28 through 39 should be 26 and 39, and citation of references 39 through 42 should be 40 through 43; 2) Page 545—References cited from 31 to 37 should be 33 to 39. (ie, reference cited as 34 should be 36, reference 7 should read 11, reference 38 should read 44), and 3) reference 44 was inadvertently omitted (Snow M, Stein C, Fallat R. Interlaboratory variability of pulmonary function tests. Am Rev Diagnostics 1983; 2:43-45).

We sincerely apologize for these errors.

Rolando Berger, M.D., F.C.C.P., and David Smith, M.D., Albert B. Chandler Medical Center, Lexington, Kentucky

Communications to the Editor